United States Department of the Interior

U.S. GEOLOGICAL SURVEY
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Anchorage, Alaska 99508

IN REPLY REFER TO:

July 15, 2013

Memorandum

To: Timothy Jennings, Assistant Regional Director, Fisheries and Ecological Services, U.S. Fish and Wildlife Service, Region 7

From: Dr. John Piatt, U.S. Geological Survey, Alaska Science Center John F. Piatt

Re: Request for Peer Review of the Species Information Section of the Kittlitz's Murrelet Listing Evaluation

The FWS in Anchorage requested a peer review of biological information on Kittlitz's murrelet (hereafter KIMU) that has been compiled by the Endangered Species Office (ESO) as part of a 12-month finding on a petition to list the KIMU as an endangered or threatened species. This information was provided to me in a 20 page document with the title noted above. Concern was raised that some of the species information being used in this finding document has not been published in peer-reviewed journals, and so I should pay particular attention to these sections. For this review, I was asked to specifically comment on: 1) whether some useful information might be missing, 2) whether any statements are not adequately supported by citations, and, 3) whether biological conclusions are well-supported in the text.

I will first address each of your concerns in summary fashion, and then provide more detailed comments with reference to specific text items. I have reviewed this document with the same rigor that I treat submissions to the Auk or Marine Ecology Progress Series, both of which I serve as an editor.

My overall assessment is that the species information review conducted by the ESO represents an enormous effort in pulling together a wide array of information, much of which was poorly known or did not exist just five years ago. I extend my compliments to the ESO for conducting such a thorough job in relatively short order. That said, I have some serious concerns about two components of the review, which are described in detail below. First, the ESO has taken surprising liberty in reinterpreting population trend information published very recently (for the very purpose of this review), which amount to a reanalysis, not a review. I believe that was inappropriate. Second, models of population trend and population viability are weak at best, owing mostly to inadequate datasets for building such models, but also to debatable and selective use of the sparing data that are available. The problem is compounded by the fact that the models rely largely on unpublished information, and no information is provided in this review which would allow anyone to evaluate decisions made in constructing these models. I recommend they be deleted from future documents.

If you have any questions, please do not hesitate to contact me. Per your request, I have appended my CV for your files. I will be conducting field work in Prince Willam Sound and the Aleutians in coming months, and would be happy to visit your office if you wish to discuss my concerns in person.

REVIEW COMMENTS

Published versus unpublished:
Your concern about use of unpublished, and here undocumented, information is probably valid. A tally of citations within the body of text shows that 18% of all citations (n=415) are to "unpublished data" and a few "in litt." sources (from outside and inside FWS). Citations by subject matter reveal that references to unpublished work are not excessive (5-14%) in the reviews of taxonomy, habitat use, foraging, and population status, but high in the reviews of demography (26%), nesting (29%), and the population model (38%). More than half the citations of unpublished information refer to one source. In the bibliography, I found that 72% of citations (excluding "in litt.") were to what is widely considered "published literature" (journals, theses, peer-reviewed agency publications), while 28% cited unpublished (and not reviewed) reports, abstracts and posters. Thus, more than a quarter of citations are unpublished and un-reviewed.

Being "unpublished", in itself, does not imply that the information is unreliable, and in the case of *available* reports anyone can evaluate data, analyses and conclusions for themselves. This is not true, however, for "in litt." and "unpubl. data" citations because those data, analyses or conclusions cannot be evaluated by anyone else but the ESO. This points to a problem I have with reviewing this document. It is a text document only, with little or no supporting information (methods, tables, graphs) with which to evaluate its conclusions. For many of the results and conclusions of the population trend analysis and the population dynamics model, all a reader can do is take it at face value. This is all the more reason why the citations themselves should not cause doubt about the quality of the information sources. For example, one of the cited reports is a highly questionable document (Day 2011, see below), and, in my opinion, should never have been included in this review.

Is some useful information missing?
This is not an exhaustive review, but it does seem to cover information that is most useful in considering population status. Little or nothing is mentioned about the inland attendance behavior of KIMU on the nesting grounds (diurnal and seasonal patterns of attendance, flyways, vocalizations, etc.), presumably because it may not be that useful for assessing current status (although it may be useful for monitoring future status). On the other hand, little mention is made of the precarious energetic situation KIMU likely find themselves in much of the time, and that may well be important and useful information. Working independently, both Agness et al. 2013 and Hatch et al 2012 concluded from modelling of energetics of KIMU that the species is living on the edge, metabolically speaking, and has little ability to cope with additional stress from, say, depletion of food supply, increased flight activity (further inland to nests, or from disturbance). This would help explain the extreme vulnerability of KIMU to various threats; and help explain why KIMU has exhibited population declines, low breeding success and low adult survival, while related species in the GOA and Bering Sea have not exhibited similar problems. Finally, a few references might be useful to augment what has been compiled (also see detailed comments below). For example, in her thesis, Agness (2006) includes an appendix on the chick diet of KIMU in different habitats of Glacier Bay, showing the importance of capelin in more glacial-affected waters.

Are statements adequately supported by citations?
Yes, for the most part, statements of fact are usually supported by a citation. However, in many cases that citation is inaccessible to the reader (some reports, all "in litt." and "unubl, data" citations), so the adequacy of that kind of citation is a different issue, discussed above and below. I have thought of a few other citations that might be included in the general review, see below. Citations referred to my review

can be found in the Literature Cited document provided to me by the FWS. If not, I have provided the entire citation here.

Are biological conclusions well supported in the text?
There are a few major areas where I think the conclusions or statements are not supported. In particular:

1) Statements about Glacier Bay survey trend are incorrect and inconsistent with published results. The ESO review states Piatt et al. (2011) reported a "local population decline of 89 percent … but the decline was not statistically significant due to high inter- and intra-annual variance". What Piatt et al. 2011 actually said was: "Kittlitz's Murrelet numbers declined by 85% in shoreline habitat and by 90% in offshore habitat between 1991 and 2008 (Table 2). Combined data indicated a bay-wide population decline of 89%. Trend analysis of shoreline transects was robust owing to good sampling effort in all years of study (Table 1) and to relatively low variances of population estimates within years (Fig.4). Linear regression analysis of log-transformed densities in the shoreline strata, weighted by the inverse of the bootstrap variance, indicated a significant decline over time ($F = 16.40$, $P < 0.0098$, $r2 = 0.77$), at a rate of -14.4% per year." "Comparison of offshore transects was less robust owing to smaller sample sizes, especially in 1991 and 2008 (Table 1), yet weighted linear regression of log-transformed densities in the offshore stratum indicated a marginally significant decline over time ($F = 5.35$, $P < 0.0686$, $r2 = 0.52$), at a rate of -10.6% per year." These facts should be reported as they appear in original documents, not reduced and filtered so much as to change their original meaning.

   ESO suggests that Piatt et al. "disproportionately sampled nearshore habitat in 1991" which "raises concerns" about comparability of the 1991 data to later years, and concern about the "reliability of the 1991 survey". I believe this is a grossly misleading characterization of these data. Piatt et al. (2011) used FWS protocols developed in PWS in 1989 by Klosiewski and Laing (1994) to survey 724 linear km of shore— almost the entire mainland coast— of Glacier Bay in 1991, with no intention of conducting a survey of the offshore, although some opportunistic surveys were conducted in that habitat, as later reported. There is nothing "unreliable" about those nearshore surveys, any more than the nearshore surveys conducted by Klosiewski and Laing (1994) in Prince William Sound (PWS) are unreliable. As stated in Piatt et al. 2011, they provide a robust and extensive survey of this habitat (comprising >20% of Glacier Bay) with which to compare over many years to follow. There is nothing "unreliable" about the offshore surveys either, it's just that few were conducted and sampling was opportunistic not random or systematic, so trend analysis is naturally weaker. Both datasets, nearshore highly significant ($p<0.01$), and offshore marginally significant ($p<0.07$), show similar magnitudes of decline (85%, 90%). Apparently, the reviewers and editors of the papers published on KIMU status and trends agree with that assessment, or it would not have been published. The use of pejorative language by ESO to describe published findings is not necessary. ESO should just report the results of published work unfiltered and without subjective statements.

2) After reviewing the evidence for KIMU declines in Prince William Sound (p. 16), including the undisputed evidence that all *Brachyramphus* murrelets (BRMU) declined by 70% over 23 years, ESO concludes that it is "difficult to draw conclusions about the status of Kittlitz's murrelet in Prince William Sound from this analysis". This statement is baffling because it conflicts with statements and conclusions presented by Kuletz et al. (2011, 2013). Both field data, and data modelled under various scenarios with careful consideration of data quality issues, show major declines of KIMU and MAMU in PWS from 1989 to 2007. ESO gives equal time to Hodges and

Kirchhoff's (2012) reinterpretation of trends, but does not bother to tell us the main reason why their analysis was quickly shown (Kuletz et al 2013) to be flawed: That in addition to adding inappropriately modified survey data that were not part of the comparable time series, they discarded surveys where KIMU were identified outside of what they deemed core areas because, in their judgement, KIMU would not be found in those areas and must therefore have been misidentified. This circular logic has proven to be completely erroneous, because not only have KIMU been resighted in those "non-suitable" areas during recent PWS bird surveys, but specimens have also been caught in gill nets in those areas (Kuletz et al 2013). Without a reason to remove those surveys, the analysis and conclusions of Kuletz et al. (2011) remain intact, and ESO should say so (if they still feel compelled to even talk about this). As Cushing et al (2013) has subsequently reported: If *Brachyramphus* murrelets declined by 70% during the past couple decades, but all that decline was due only to marbled murrelets, and KIMU were as abundant today as they were 20+ years ago, then about 1 out of every 3 murrelets observed in PWS today would have to be a KIMU. This conclusion does not "hinge on comparable identification rates", as suggested by ESO. The documented fact, and my personal experience, is that in recent years only about 1 in 20 murrelets observed in PWS is a KIMU. You can't have massive declines in BRMU, all due to MAMU only, without a correspondingly massive change in the ratio of KIMU to MAMU. That didn't happen, and KIMU must therefore also have declined. There is no other possible explanation. In summary, I think that ESO should place more positive emphasis on the careful consideration of trends in PWS given by highly experienced FWS biologists like Kuletz and Irons, and less on the demonstrably flawed interpretations of two critics who apparently lack basic knowledge about the ecology of this species in PWS.

3) The analysis of an "all population" trend (p. 18) is flawed, and therefore the biological conclusions are as well. It is not clear what data ESO combined in other areas, but for Glacier Bay ESO combined results from Piatt et al. (2011), Kirchhoff et al (2013; unpublished, un-reviewed, unavailable), and Hoekman et al. (2013). This approach was roundly rejected by the expert statistical reviewer of the Piatt et al. (2011) Glacier Bay paper, and the ESO is aware of that, so I am quite surprised to see it again here. Protocols, sample design, timing of sampling and analysis methods differ greatly among these 3 survey designs, making integration of all three a statistical *faux pas*. This is true even if you could apply correction factors for the gross differences in methods, and I see no evidence that was attempted here. See more details below. ESO also made adjustments to Prince William Sound and Kachemak Bay data, making choices about which data to drop based on their own judgements about data quality. The ESO then comes to the conclusion that there is no significant change in total population, and that the population was "stable between 1989 and 2012". This is a biological conclusion that is not only unsupported by the data, it flies in the face of the published analyses conducted by the professionals who actually collected and analyzed those data for each of those sites.

   The argument put forward for combining the analyses of data from these three sites is that the "apparent trend in local population size of the Kittlitz's murrelet is confounded by intra and inter-annual movements of individuals among study sites". As evidence, ESO refers to unpublished data from Icy Bay, which actually suggests an extremely low rate of emigration. No data is available on immigration rates. No evidence is presented to show that there is any exchange between the 3 study sites of Glacier Bay, PWS and Kachemak Bay, although satellite tag data suggest post-breeding movements from east to west (Madison et al. 2012). The extreme example of annual variability provided for Kachemak Bay is just that, extreme, and in no way mirrored by the annual variability observed in Glacier Bay or PWS. In short, there is no reason

why we should suspect a large influx of birds during the census period in any given site is a result of emigration from one of the other sites, and therefore no compelling reason to combine trends from all sites to do a "population analysis".

Thus it appears that ESO has conducted an analysis which does not have statistical value, nor even heuristic value. And in the course of doing so, the ESO has discarded the conclusions of published population trend analyses developed with statistical rigor by the investigators who conducted the surveys, and replaced it with a conclusion that the KIMU population was "stable between 1989 and 2012". In my view, this is grossly misleading. The actual datasets and analyses ESO conducted using these data are not presented, and nothing about the analysis here is transparent or repeatable. With a similar lack of transparency, ESO draws a final conclusion that the "marbled murrelet across all populations indicated a stable population trend from 1989 to 2012". This is an astonishing conclusion given the massive and well documented decline of *Brachyramphus* murrelets in Prince William Sound, Glacier Bay, and adjacent areas (Piatt et al. 2008, Cushing 2013) which, even if you assumed was only due to a decline in KIMU populations, could not conceivably account for the massive decline in BRMU numbers in each location. It is paradoxical to report on the one hand that *Brachyramphus* murrelets in the core of their range have declined by 70-80% over 20+ years, while suggesting on the other hand that *both* KIMU and MAMU have had stable populations during that same time. For clarity, and accuracy, I would drop the so-called population trend analysis and simply report the full results presented by authors in the status and trend reviews of KIMU in the important population areas.

4) A population model (p. 19-20) was used to link population trends with demographic rates. Unfortunately, this suffers from a similar lack of transparency. Since none of this is published, and none of the methods, assumptions, models, statistics and results are available to examine here, it is difficult if not impossible to "review" this section of the document. Here, I will provide some comments on the model, based more on my participation in a review panel assembled last winter to review a Powerpoint presentation of the model. Now, as then, I have some serious reservations about the choice of parameters made to develop this model.

   Trend data: Only data from 2000 to 2012 were used in the model "because only abundance data was available prior to 2000 and without concurrent reproduction or survival data we were unable to achieve good model fit". However, the choice to exclude the time period from 1989 to 2000 removes all data showing the massive decline in numbers, leaving only the later period when the decline had leveled off in all datasets. Most of the reproduction data are from Agattu and Kodiak, and most of that was collected between 2008 and 2012. The next largest dataset on breeding came from SE Alaska, collected between 2007 and 2012. Clearly, the breeding success data still do not mesh with the time series from 2000-2012, so using "data synchrony" as an argument for excluding earlier data is disingenuous.

   Breeding propensity: By the ESO account, the radio telemetry data suggest breeding by only 18% of the population, and brood patch information is a notoriously unreliable indicator of breeding, but ESO uses it to estimate that as much as 82% of the population is breeding. This then plugs into the model as a high end estimate of propensity, and has a strong influence on the outcome of the model, which is potentially very misleading.

   Survival data: ESO quotes unpublished data on survival of radio-tagged birds during summer, suggesting a survival rate of 0.89 over a 60 day period. But for all other alcids, most mortality

occurs during winter, so this summer rate is just a fraction of annual mortality. If this rate were applied to 365 days, which may or may not be appropriate, then annual survival would be only 49%. That would be an extraordinarily low rate of survival, and you would think cause for at least some mention here and sober reflection. From mark-recapture banding results, ESO estimated annual survival at about 80%, with a confidence range of approximately 14% to 100%. But for the model, the ESO uses low, medium and high estimates of 0.79, 0.89 and 0.95 respectively, with the latter high value pulled from *a possible* MAMU range and having no basis in reality for KIMU. I should think a more reasonable range would be low 0.49 and medium 0.80 based on Icy Bay data, and 0.86, an average of the 2 best field estimates for MAMU (0.83 and 0.88).

This is a weak model, not for lack of trying, but for lack of reliable data collected over the same time and space, and the conjunction of unverifiable assumptions used to string together the model. Is even 1 of the 7 model assumptions made on p. 20 true? How would we know? In the end, the model uses key survival data from one site, breeding success from 2 sites not included among population trend sites, excludes trend data from the period when most population change occurred, uses demographic data from the late 2000s to interpret trends in the early 2000s, and employs at best unreliable, at worst wildly inaccurate, high end estimates for breeding propensity and survival which of course have an enormous influence on the model outcome. All of this is provided here without documentation as to specifics of how data were handled, model options used, etc. Despite all this, we are led to the final (p. 20) conclusion "that the all-population model described here, and to a lesser extent, the trend analysis, provide the most reliable source of information on the rangewide status of Kittlitz's murrelet". "We conclude that Kittlitz's murrelet populations likely declined prior to 2000 but the magnitude of the decline is not known with reasonable certainty. Since 2000, the populatons appear to have stabilized or may be declining at a slow rate..".

I would agree with only the latter statement. The leveling off of the decline in the early 2000s (probably after 2002) has been noted already (Piatt et al. 2008, 2011; Kuletz et al. 2011, Cushing et al. 2013).

The models, by predetermination of which data to use, and which assumptions to make, fail to describe or capture the early decline of KIMU, and are therefore quite irrelevant to the issue of major declines in populations. The conclusion that these models "are the most reliable source of information" on the status KIMU is not supported by the data. The models, by necessity, are built upon incomplete data and "guesstimates". Truth is, these models are useful for exploratory purposes, but tell us more about what we need to learn than about what we want to know.

The magnitude of the decline prior to 2000 is not known with reasonable certainty? I am dumbfounded by that statement. A brief perusal of papers published on population declines of the 1990s indicate both magnitude and levels of certainty. I find it disturbing that a modelling exercise is being substituted for results of hard data analysis. As noted above, I recommend that the model should be dropped from this listing document, and the data on trends presented in original status and trend documents should be used to discuss population trends.

Specific comments:

p. 2. Distribution. Occupies "vast area", but populations concentrated in relatively few sites and during breeding season mostly in sheltered inside areas like PWS or GLBA, or within a few km of shore.

p. 3. Line 3. Murrelets move to breeding areas in March or April. While some birds may actually stay year round (thus showing up on March surveys), it appears that bulk of birds don't show up en masse until May, even mid to late May, according to Beardslee Island surveys at GLBA, and surveys of PWS (see Stephenson 2009).

p. 3 bottom. I would say .." murrelets are *usually* but not exclusively associated with glacially influenced waters, *often* those with floating ice…"

p 4. On the marine habitat side, I would say that KIMU associated with glacially modified waters (colder, lower salinity, stratified, turbid) which appear to support an abundance of important KIMU prey including euphausiids and capelin (Arimitsu, Renner refs).

p.4. end first paragr. "species occupies and feeds in marine waters with and without glacial influence". Yes, but in Alaska this comprises a small proportion of the population during the breeding season. Vast majority of the population is foraging within influence of glacial marine system.

p.4 diets. Missing very important references of Sanger (Sanger, G. A. (1986). Diet and food web relationships of seabirds in the Gulf of Alaska and adjacent marine regions. OCSEAP Final Report, 45) and Agness, A. M. (2006). Effects and impacts of vessel activity on the Kittlitz's Murrelet (Brachyramphus brevirostris) in Glacier Bay, Alaska. MSc. Thesis University of Washington.

p.4. 3rd paragr. "the KIMU is a flexible forager". Not really. The Tufted Puffin is a flexible forager, eating just about any forage sized fish throughout its ranges, including more than dozen common species and up to 80 different taxa. Despite its wide distribution, the KIMU eats almost entirely sand lance, capelin, herring and euphausiids, and just a few other items in small amounts. It probably has a need for very high quality forage (Hatch 2009, Agness et al. 2013).

p.5. chick diets. Capelin almost certainly underestimated as all studies have been conducted post regime shift when capelin in the GOA, except in GLBA, a refugium for capelin (Arimitsu et al. 2006), where capelin were dominate fish fed to chicks in cool, glacially modified waters in the upper bay (Agness 2006). Kelp greenling and Atka mackerel are almost certainly an anomaly, as sand lance are the usual prey for seabirds at Agattu, and chicks fed these prey had very poor growth, so probably not the choice parents would make if sand lance were available.

p.5.parag 2. KIMU may be found in some areas with high tidal currents, but overall tend to forage in areas with slow currents, and choose times to forage when currents are slack (Drew et al. 2013, Effects of currents and tides in fine-scale use of marine bird habitats in a Southeast Alaska hotspot. Marine Ecology Progress Series, *In press*. (online).

p.6. parag 3. KIMU may be able to use more vegetated sites in Aleutians because (under normal circumstances) they are less likely to contain or attract mammalian predators.

p. 8. 2nd parag. Chicks fed 1-12 times per day, but this range and variation in part due data collected at

sites where birds obviously struggling to raise young (judging from growth rates). A rate of probably 4 fish per day would be more typical for a fish-eating alcid in a normal food supply year.

p.9. 2nd parag. Chick death (starvation, exposure, or disease 29 %) I would add saxitoxin to that list of cause of death.

p.10. end, 1st parag. Faster fledging at Kodiak likely due to better quality food, faster growth.

p. 13. Status and trend. End, parag. 1, reference to Day 2011. (Day, R.H. 2011. Evaluating population trends of Kittlitz's Murrelets in Alaska. Final Report to Alaska Department of Fish and Game, Juneau, Alaska. Unpublished report. ABR, Inc.—Environmental Research and Services, Fairbanks, Alaska. 60 pp.). I find it disturbing that this unpublished and un-reviewed document is being presented as an authoritative source of information about the quality of the data and interpretation. I don't have time to delve into the details, but here's a few statements taken from longer documents delivered to the ADF&G after they released their report: On October 11, 2011 the FWS Region 7 Director wrote that the report contained "inaccuracies and/or mischaracterizations of research conducted by the Fish and Wildlife Service". In subsequent correspondence, Dr. Kathy Kuletz (MBM) stated "The Day report suffers from lack of review to correct inaccuracies, incorrect references to unfinished reports, and unsupported statements and/or speculative statements". In a letter to Day (Sep. 8, 2011), Dr. Brendan Moynahan said "I find your report contains numerous mischaracterizations of our efforts to monitor murrelet abundance, distribution and trend in Glacier Bay National Park", and proceeded to reproach Day for his "faulty criticisms" and sent a second document that "details specific instances of faulty criticisms". In my own letter to ADF&G, I noted that "errors and misleading characterizations of data or its interpretation are found throughout the report". Further, while the report is supposed to be an evaluation of population trends of Kittlitz's murrelet in Alaska, "there is not a single analysis of data here, or test of data bias, or statistical comparison of data, or statistical evaluation of trend. Its rather strong conclusions are based entirely on Dr. Day's personal interpretation of other peoples' work". When a report such as this can be so roundly criticized by those intimately familiar with the issues, and yet be used here, it casts doubt on credibility of the rest of this listing document. Indeed, I see that some of Day's unsubstantiated claims are simply repeated by ESO here.

p.13. last parag, "the high spatial variability of KIMU often results in high variances associated with population estimates and little power to detect trend (Kissling et al,. 2007)". This overstates the difficulty of surveying this marine bird, which, as marine birds go, has a relatively low CV when sampled using standard FWS transects. The citation referenced states (p. 2186) "Power to detect an annual decline of 10% increased rapidly and reached 1.0 in just 10-15 years". In PWS and Glacier Bay where rates of decline exceeded 10% per year, and surveys were conducted over 17-23 years, declines should have been easily detected. And they were. In fact, a study examining 5 years of survey data comprising 5300 km of transects in Glacier Bay (Drew et al. 2008) revealed that KIMU had among-transect CV's that were less than almost all other common species, including other alcids, gulls, waterfowl, loons, cormorants and terns. Power analyses on Glacier Bay transect data of the eight most common species showed that a 7% per year reduction in KIMU would likely be detected in about 10-11 years. This was greater power to detect change than observed for kittiwakes, mergansers, seals, harlequin ducks, scoters, goldeneye and Steller sea lions; but less than observed for pigeon guillemots, perhaps the most dispersed seabird in the bay. [see Drew. G.S., S.G. Speckman, J.F. Piatt, J.M. Burgos, and J.L. Bodkin. 2008. Survey Design Considerations for Monitoring Marine Predator Populations in Glacier Bay, Alaska: Results and Post-hoc Analyses of Surveys Conducted in 1999-2003. USGS Final Report for the National Park Service, USGS Alaska Science Center, Anchorage, Alaska. 153 pp.]

p. 13. Last parag. It is true that in earlier surveys, at Glacier Bay at least, a higher proportion of murrelets were unidentified. But this does not mean that the positive IDs made were incorrect, and unidentified birds can be pro-rated to each species. This is a procedure routinely done in marine bird and mammal surveys, where many similar sister-species make identification challenging (e.g., Common and Thick-billed Murres, Short-tailed and Sooty Shearwaters).

p. 13. Last parag. Another issue, not mentioned here, but surely as important to mention as the mis-identification alleged by Hodges and Kirchhoff (2012) is surveys in which *no* murrelets are unidentified because survey protocol called for that (i.e., Lindell 2005). This led to another serious issue of misidentification, where observers presumably felt compelled to ID birds that were not possible to ID, and tended to ID them as the more common species (i.e., MAMU). As a result, KIMU were reported at more than a 3x lower rate than observed in other years, a statistically improbable result (Piatt et al. 2011).

p. 14. 1st parag. Again, the ESO does a disservice to itself and anyone else reading this document by quoting Day et al. as an authority on how well historical FWS-MBM surveys can detect population declines.

p. 14 last parag. "few surveys were conducted prior to 2000 and reliability of those data are compromised due to the methodological challenges to surveys that are presented above". Again with the judgmental re-evaluation of trend data recently analyzed by experts. I don't believe that you will find this kind of statement in Kuletz et al. 2011a, b,; or Piatt et al. 2011, or Cushing et al. 2013, or Drew and Piatt 2008, and it will almost certainly be challenged if it appears in final listing documents.

p. 17. 2nd paragraph. Noting the temporal differences among surveys, FWS states: "the timing of the four surveys varied dramatically, especially between the 1993 survey (7-23 June) and the 1996-1999 surveys (14 July -16 August; p. 87), severely reducing the comparability of these surveys across years." This concern is valid, because murrelet numbers in July and early August tend to be much higher than those observed in June (Romano et al. 2004, Stephenson 2009, Kuletz et al. 2011). This is probably why the calculated rate of decline actually increases from 26.2% per year to 32% per year when you drop the 1993 data. I don't think I would call this misleading, however, since the authors actually point out the issue and analyze the trends with and without the 1993 data. In contrast, I would say that it is misleading to assess trends of KIMU in Glacier Bay by combining surveys conducted mostly in June by Piatt et al. 2011 with those conducted in July and August by Kirchhoff et al. 2013 and Hoekman et al. 2013, and not mention this important issue (as done in the population trend analysis on p. 18) nor make any attempt to compensate for it (by removing data as in Kuletz et al 2011, or adjusting values).

p. 18 last parag. In addition to the timing of survey issue mentioned above, FWS also fails to mention that Hoekman's survey protocol differs dramatically from other protocols (surveying more in known high density areas) and uses new algorithms for calculating abundance that yield estimates that are 2 or 3 times higher than everyone else. It may be these protocols and analyses are better, and more accurate, but surely we should examine the ramifications of adopting new survey/analysis protocols before we start comparing survey results?

United States Department of the Interior

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IN REPLY REFER TO:
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July 13, 2013

Dear Mr. Jennings,
Thank you for the opportunity to review the Species Information Section (the Section) of the Kittlitz's Murrelet Listing Evaluation as part of the Endangered Species Act. I have provided a number of comments and specific editorial suggestions directly on the draft using Track Changes in MS Word. While I felt this was the most efficient way for Michelle to address those comments and accept/reject the editorial suggestions, I'm happy to provide a copy of the list of comments in a separate document referencing the Sections' page number if needed.

A few notes about the review. First, while I have a general, working knowledge of the ecology and life-history of Kittlitz's murrelets, there are certain aspects of the Section, such as the population genetics component, of which I feel unqualified to review as I have only a peripheral understanding of the state of the science. Second, I tried to review the document focusing on the three general attributes described in your cover letter including (1) Whether any additional relevant biological information should be considered; (2) The degree to which the conclusions are supported in the text; and (3) Whether any statements require additional citations. Although not explicitly stated, most of my comments are generally focused on addressing one these three attributes. Finally, I have never reviewed a draft Biological Section and thus it's difficult to evaluate whether the Section represents a comparatively in-depth and well-written review and whether my comments requesting more information is commensurate with the scope of what a Species Information Section is expected to provide. In other words, a number of my comments suggest that the Section would be strengthened by providing more information by (a) putting certain rates into context by drawing on relevant, comparative information for other similar but better-studied species, and/or (2) providing more detailed information on the models used in drawing the conclusions. For example, listing adult or nest survival rates, while helpful, are particularly informative when placed in context of what is known about these rates from other species, including whether they are sustainable in the immediate or long term. Likewise, the need for more information about the models used to draw the conclusions about rates of decline is fundamental to understanding their relevance, accuracy, and ultimately their utility. Perhaps model output, diagnostics, and other details are listed in other Sections of the Listing Evaluation, or are far beyond the level of detail that the Section is expected to provide. Without seeing the other Sections it's difficult to say whether more information is needed in the Biological Section; however, without more details, it's difficult to evaluate the degree to which the conclusions are supported.

I hope you find the review helpful in generating an informative Listing Evaluation. Please let me know if there are any questions pertaining to my review comments. Otherwise I will communicate directly with Michelle on any clarifications.

Sincerely,

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Cc: Sonja Jahrsdoerfer
Cc: Lisa Etherington